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THE FALL
OF THE
BIRTH-RATE

A PAPER READ BEFORE THE CAMBRIDGE
UNIVERSITY EUGENICS SOCIETY,
20 MAY 1920

BY

G. UDNY YULE, M.A.
UNIVERSITY LECTURER IN STATISTICS

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THE FALL OF THE BIRTH-RATE

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PREFATORY NOTE

THE following paper was written for an audience the majority of whom could be expected to possess little or no previous knowledge of the subject. My endeavour was therefore rather to give an exposition of matters already known and views previously expressed than to add anything novel. The paper has been reprinted in the hope that such an exposition may be of service to others. The form in which it was delivered has been retained, with the exception of minor corrections: to explain a phrase which occurs here and there it may be added that copies of the tables were in the hands of the audience.

G. U. Y.

ST JOHN'S COLLEGE,
July, 1920.

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THE FALL OF THE BIRTH-RATE

On the 26th of October, 1769, Mr James Boswell after dining with Dr Johnson at the Mitre went home with him to tea. "Russia being mentioned as likely to become a great empire, by the rapid increase of population: JOHNSON. 'Why, Sir, I see no prospect of their propagating more. They can have no more children than they can get. I know of no way to make them breed more than they do. It is not from reason and prudence that people marry, but from inclination. A man is poor; he thinks, "I cannot be worse, and so I'll e'en take Peggy."' BOSWELL. 'But have not nations been more populous at one period than another?' JOHNSON. 'Yes, Sir; but that has been owing to the people being less thinned at one period than another, whether by emigrations, war, or pestilence, not by their being more or less prolifick. Births at all times bear the same proportion to the same number of people.'"

To me this remarkable dictum appears to be contradicted by the experience of every nation for which we have records over a sufficient period of time and of sufficient accuracy. It is still, however, the prevalent view that, in the course of nature, legitimate births at all times bear the same proportion to the same number of married persons of fertile ages, and that any alteration in this proportion implies artificial control. This view will be traversed in the latter part of my paper. First let us consider the data respecting births in our own country, England and Wales, and endeavour to interpret the figures.

Civil registration of births—as distinct from registration by the clergy—was not established in England until 1837, when the General Register Office was founded and the post of Registrar General created. Registration was not, however, made compulsory until the passing of the Births and Deaths Registration Act of 1874. Under the Act of 1836 it was the duty of the local Registrar to "inform himself" of the birth, and he could require answers, but it was not the duty of the parents, relatives, or other persons to go to him and give him information. By the Act of 1874 births had to be registered within 42 days under penalty.

TABLE I.

England and Wales, Annual Birth, Marriage and Death-rates; the death-rates in 1915 to 1918 are based on civilian deaths and estimated civilian population.

Period	Births per 1000 living at all ages	Persons married per 1000 living at all ages	Deaths per 1000 living at all ages	Deaths of infants under one year per 1000 births
1851–55	33·9	17·1	22·7	156
1856–60	34·4	16·7	21·8	152
1861–65	35·1	16·8	22·6	151
1866–70	35·3	16·4	22·4	157
1871–75	35·5	17·1	22·0	153
1876–80	35·3	15·3	20·8	145
1881–85	33·5	15·2	19·4	139
1886–90	31·4	14·7	18·9	145
1891–95	30·5	15·1	18·7	151
1896–00	29·3	16·1	17·7	156
1901–05	28·2	15·6	16·0	138
1906–10	26·3	15·3	14·7	117
1911–15	23·6	16·4	14·3	110
1916	20·9	14·9	14·4	91
1917	17·8	13·8	14·4	96
1918	17·7	15·3	17·6	97
1919	18·5	19·7	13·8	89

This imperfection of the earlier system and the fact that compulsion was not enforced until 1875 should

be borne in mind in considering the data published by the Registrar General for England and Wales, from whose reports the data of Table I are extracted.

It will be seen from the first column that while in the quinquennium 1851–55 the births registered were in the proportion of 33·9 per thousand of the population per annum, this figure rose to a maximum of 35·5 for the quinquennium 1871–5 and since then has fallen without a break, the average for 1906–10 being only 26·3 or rather over 25 per cent. less than the biggest quinquennial average. The greatest figure touched in any individual year was 36·3 in 1876, while in 1911 it was only 24·4, a fall of roundly one-third. To the rise previous to 1876 too much importance cannot be attached, as it is uncertain how far it may be due to increasing completeness of registration: it is with the fall I am mainly concerned. I have added to the quinquennial averages the figures for single years up to 1919, but later years are so largely affected by the special circumstances of the war that I propose to confine myself for the most part to the pre-war period ending with 1911—the year of the last census.

Compare first the change that has taken place in the birth-rate with the changes that have taken place in the marriage-rate and in the death-rate. You will see that the marriage-rate presents no conspicuous fall comparable in magnitude with that of the birth-rate. From 1851 to 1875 it fluctuated round a figure of 16 to 17 persons married per thousand of the population per annum, then fell rather abruptly, and has since fluctuated round 15 to 16 per thousand, only falling under an average of 15 during the quinquennium 1886–90. The marriage-rate shows, then, a fall approximately coincident in time with the beginning of the fall in the birth-rate, but this fall did not continue.

The death-rate (persons dying per 1000 of the population per annum) has fallen continuously since the quinquennium 1861–5, when it stood at 22·6, to the low value of 14·7 for the quinquennium 1906–10, a fall of over one-third. Owing to this fall, the "natural rate of increase" of the population, given by the excess of the birth-rate over the death-rate, has not dropped so much as might have been expected. It averaged 12·0 per thousand per annum for the decade 1851–60, rose to a maximum of 14·0 for the decade 1871–80 and fell to 11·7 in the decade 1891–1900 and 11·8 in the following decade. The greatest quinquennial average reached was 14·5 in the quinquennium 1876–80. As a general rule a lower birth-rate is accompanied by a lower mortality in infancy. It is therefore rather remarkable that the infantile mortality—which, as we do not know the numbers living under one year of age with any accuracy, is usually measured by the number of deaths under one year of age to 1000 births in the same year—did not show any effective decrease until after the beginning of the present century. Since the year 1900 there has been a very considerable saving of infant life.

We have seen then from Table I that:

(1) There has been since 1876 a heavy fall in the birth-rate amounting to roundly one-third.

(2) This has been accompanied by a countervailing fall in the death-rate (involving since 1900 a considerable saving of infant life) so that there was still in both the last two intercensal periods a natural rate of increase of over 11 per thousand per annum, or rather more than 12 per cent. in a decade.

It is clear, however, that the death-rate cannot fall indefinitely, while there is nothing to stop the birth-rate from falling to zero, so that an increase in the

future cannot be certain if the birth-rate continue to decrease.

(3) The marriage-rate showed an abrupt, but not a persistent fall. In so far as this goes, it suggests that the fall in the birth-rate is due rather to a fall in the productivity of married couples than to a fall in the proportion of married couples to the population.

TABLE II.

Decrease in the birth-rate in various countries; data from *Statistique Internationale*.

Country	Average annual births per 1000 at all ages		Decrease per cent. of the rate in 1901–10 on 1871–80
	1871–80	1901–10	
England	35·4	27·2	23
Scotland	34·9	28·4	19
Ireland	26·5	23·3	12
Denmark	31·4	28·6	9
Norway	31·0	27·4	12
Sweden	30·5	25·8	15
Finland	37·0	31·2	16
Austria	39·0	34·7	11
Switzerland	30·7	26·9	12
German Empire	39·1	32·9	16
Netherlands	36·2	30·5	16
Belgium	32·3	26·1	19
France	25·4	20·6	19
Italy	36·9	32·7	11
Serbia	40·5	38·9	4
Australia	36·1	26·5	27
New Zealand	40·5	26·8	34

Before dealing with this last point in more detail, turn to Table II which shows how widespread—how almost universal—is the phenomenon with which we have to deal. The table compares the average birth-rate for the decade 1901–10 with that for the decade 1871–80 and gives the percentage decrease; in no case cited in the table is there an increase. The fall in our own country (23 per cent.) is only exceeded by that in Australia (27 per cent.) and in New Zealand

(34 per cent.). If we want to find countries where at all a conspicuous increase has been recorded in the course of the two or three decades preceding 1901–10 we must turn to Japan or Ceylon. In the case of some registration areas in the U.S.A. there is approximate steadiness or slight increase, as also in the case of some of the states of South America, but immigration of more fertile stocks is probably in great part responsible in these cases. Enquiry would also be necessary whether improvement in registration had not had an appreciable effect. England, has, then, only shared in a movement in which nearly all European stocks seem to have taken part. It may be added that a chart of the annual values suggests a turning point in most countries about 1876.

TABLE III. ENGLAND AND WALES.

Changes in the proportions, etc., of married women to the population.

Year	Married women of all ages and under 45 per 1000 of the total population		Of 1000 married women under 45 the proportion under 35 was
	All ages	Under 45	
1851	168	112	603
1861	174	116	600
1871	174	115	607
1881	171	113	604
1891	170	112	597
1901	176	117	593
1911	184	119	559

Returning now to the question how far the decrease is due to a fall in the fertility of married couples, and how far it may be ascribed to a decrease in the proportion of married couples to the population, or of course to an increase in the average age of wives, Table III gives some simple information on this head for the census years since 1851. It is only for these

years that we have precise information on the point. It will be seen from the first column that the proportion of married women to the population fell from 1871 to 1891, but since then has *risen very considerably*. We may add to our conclusions then:

(4) The fall in the birth-rate, since 1891 at least, cannot be due to a fall in the proportion of married women to the population, since that proportion has risen, not fallen.

But, it may be said, married women are relatively infertile after the age of 45, and probably there were fewer under that age in 1911 than in the seventies, as compared with the whole population. The answer to this objection is given in the next column of the Table. The proportion of married women under 45 to the whole population fell from 1861 to 1891, but rose from 1891 to 1911, when it stood at the highest figure for the period. We see then that

(5) The fall in the birth-rate cannot be due to a fall in the proportion of married women of fertile ages to the population, for this proportion has risen since 1891, and now stands at the highest point since 1851.

These changes may seem to be of a rather unexpected kind having regard to the fact that there was a clear fall in the marriage-rate following the decade 1866–75. But the increase in the proportion of married women to the population after 1891 is in part due to the fall in the birth-rate itself. This leads to a fall in the proportion of the young, and therefore to a rise in the proportion of adults (including married women) as compared with the population as a whole.

But, our imaginary arguer may then say, perhaps, of those under 45 there is now a much smaller proportion at the younger ages, and therefore the group of married women under 45, considered as a whole, cannot be as fertile as it used to be. On this head he

would be right, as shown in the last column of Table III. Of 1000 married women under 45 years of age in 1871 no less than 607 were (or said they were) under 35 years of age, but this proportion fell to 604 in 1881, 597 in 1891, 593 in 1901 and 559 only in 1911.

We must then add to our previous conclusions the modifying clause:

(6) The effect of the increasing proportion to the population of married women of fertile age will have been lessened, or possibly nullified, by their increasing average age.

Now let us eliminate from the birth-rate in some way the effect of these changes in the proportion of the married women to the population and in their average age.

TABLE IV. ENGLAND AND WALES.

Legitimate birth-rate and fertility of married women.

Year	Legitimate births per 1000 living	Legitimate births per 1000 married women under 45	Tait's coefficient	Newsholme and Stevenson* standardised legitimate birth-rate
1851	31·9	285	1·66	33·06
1861	32·4	280	1·63	32·36
1871	33·0	288	1·67	33·12
1881	32·2	285	1·66	32·73
1891	30·1	269	1·58	31·25
1901	27·4	234	1·39	27·40
1911	23·4	196	1·20	23·67

* The figures for 1851 and for 1911 have been added to those given in Newsholme and Stevenson's paper.

First of all we must remove the illegitimate births from the birth-rate. From 1851 to 1865 the illegitimate births numbered some 2·2 per thousand of the population. A fall then set in, and from 1901 to 1910

the average was only 1·1. The legitimate birth-rate is therefore only less than the total by 2·2 to 1·1 points; the figures for the census years are given in the first column of Table IV. Taking 1871, the last census year before the fall began, as our reference point, the first column of the following table shows that the fall between 1871 and 1911 was in the proportion of 1000 to 709, a fall of 29·1 per cent.

TABLE V.

The figures of Table IV reduced by taking the data for 1871 as 1000 in each column.

Year	Legitimate births: per 1000 living	Legitimate births: per 1000 married women under 45	Tait's coefficient	Newsholme and Stevenson* standardised legitimate birth-rate
1851	967	990	994	998
1861	982	972	976	977
1871	1000	1000	1000	1000
1881	976	990	994	988
1891	912	934	946	941
1901	830	812	832	827
1911	709	681	719	715

The next possible step is also clear. Instead of taking the ratio of legitimate births to the population, take the ratio to the number of married women between 15 and 45 years of age. This will at once eliminate the effects of all changes in the proportion of married women to the population, and of their ages, except in so far as they have altered between the age-limits given. The numbers of births in the census years per thousand married women aged 15 to 45 are given in the second column of Table IV. They fell from 288 per thousand in 1871 to 196 only in 1911, and the proportional figures in the following table show that this is in the ratio of 1000 to 681.

Now we know that this figure must be too low, for we have seen from Table III that the average age of the married women under 45 was appreciably higher in 1911 than in any of the years preceding it. So that the fall in the fertility of married women, apart from any effect of changes in age, was certainly less than 31·9 per cent. But how much less? Can we get any measure of this?

If, at the time a birth was registered, the age of the mother had to be given, there would be no great difficulty. We could see, at each census year, or for an average of years round each census year, how many births there had been per thousand wives aged 15 but under 20, 20 but under 25, 25 but under 30, and so on for every age-group, and we could compare the fertilities at each separate age-group from census to census. If we wanted to obtain some sort of summary or average figure for wives of all ages, we could take some fixed arbitrary numbers of wives at each age as a standard—say the numbers at the census of 1911—and calculate the numbers of births there would have been to such wives at the fertility rates for the several age-groups given by the birth-records of 1851, 1861, 1871, etc. These figures, expressed as a proportion per thousand of the population would have given us what are termed "standardised fertility rates" or "standardised birth-rates"—rates which are appreciably freed from the effect of changing numbers and ages of the married women, and are therefore measures of changing fertility. The process is analogous to the process of "standardisation" of death-rates which has long been in use.

But we have not got, in this country, the ages of the women at the birth of their children. We must therefore use some other process, and again a method already in use for death-rates is available, sometimes

called the "indirect" method of standardisation. We take any data that may be available—from any source whatever—giving the numbers of births per thousand married women at successive age-groups, hoping, however, that even if they represent a rather higher or lower average fertility the form of the law (the change in fertility with age) is approximately the same as for our own women. We then apply these arbitrarily chosen fertility rates to the populations of married women at each successive census and thus calculate the birth-rate there would have been (reckoned on the total population) if our married women had exhibited the standard fertilities. These "potential" or "index" birth-rates are simply measures of the favourableness or otherwise of the constitution of the population for the production of children. Taking some fixed year as the standard year of reference, the ratio

$$\frac{\text{index birth-rate in standard year}}{\text{index birth-rate in any other year}}$$

gives a factor for the correction or standardisation of the birth-rate in that other year. For if the index birth-rate in that other year is low compared with the index birth-rate in the standard year, its population must have been unfavourably constituted for the production of children—there must have been few married women in the population, or they must have been relatively old—and to correct for this we must have to multiply by a factor greater than unity, as above. Conversely, if the index birth-rate is high, the population is favourably constituted and the correcting or standardising factor must be less than unity.

This process of correction (as it used to be termed) or standardisation (to use the modern phrase) was first discussed and applied by Dr Newsholme (now

Sir Arthur Newsholme) and Dr Stevenson in 1905 (refs. 4, 5*) and slightly later by myself (ref. 6). Drs Newsholme and Stevenson used for their standardising fertility-rates figures obtained for Sweden in 1891, and for the fixed index birth-rate the index birth-rate for England and Wales in 1901.

I used fertility data for Glasgow and Edinburgh in 1855. This was the first year of civil registration in Scotland, and the age of the wife was then required to be returned at the time of the registration of the birth. This requirement was afterwards dropped, so that we have no later data. Dr Matthews Duncan in 1866 had the data reduced for the two towns mentioned (ref. 1): the data were never officially compiled and were not reduced as a whole till 1906 (Lewis, ref. 3). Dr Duncan handed over to Professor Tait the reduced data, and Professor Tait contributed some very interesting mathematical chapters to the book. He pointed out that an extremely simple law seemed to express, within the limits of error, the fertility of these married women:

"The percentage of wives of any one age who are mothers within one year varies directly as the difference between that age and 50."

In symbolic form, if f_t is the percentage of wives of age t who are mothers within one year

$$f_t = k\,(50 - t).$$

For the Edinburgh and Glasgow women Professor Tait found k, which may evidently be treated as a kind of coefficient of fertility, to be 1·5. Clearly this law is no more than a rough approximation to the facts (cf. the discussion in the paper cited) but it is an exceedingly convenient one to assume for working purposes on account of its simplicity, and it seems to

* See list of references at the end of the paper.

me to provide quite sufficient accuracy. Further, it avoids the necessity for going on to the second stage of calculating a standardised birth-rate, since k is itself a measure of fertility, and birth-rates standardised on the basis of Tait's law must bear the same proportions to each other as the corresponding coefficients k. We may therefore give "Tait's coefficient" only instead of the standardised birth-rates, and the calculation of the coefficient is very simple. Putting k equal to unity in the formula, find the values of f_t for the central years of the age-groups available; using these values of f_t, calculate the births that would be given by the married women. The ratio of the actual (legitimate) to the calculated births is the value of k.

For some years quinquennial age-groups are available in the English Census, but for others the age-groups are

15 but under 20
20 „ „ 25
25 „ „ 35
35 „ „ 45

To secure comparability the same age-groups must be used throughout, and therefore this was the grouping used, though a quinquennial grouping would have been better. The values of $f_t/100$ for calculating k are 0·325, 0·275, 0·20, 0·10. Thus for Connaught in 1911:

Age group	Married women	Coefficient	Calculated births
15–20	89	0·325	29
20–25	1,626	0·275	447
25–35	15,306	0·20	3,061
35–45	22,059	0·10	2,206
			5,743
		Actual births	13,697

$$k = \frac{13,697}{5,743} = 2·385.$$

In the last columns of Table IV are given the

values of Tait's coefficient, so calculated, for the census years, and also (for control) the values of the standardised legitimate birth-rates as calculated by the method of Sir A. Newsholme and Dr Stevenson. Calling the respective values in 1871 1000 we can compare the results of these two methods. It will be seen that they never diverge by as much as 1 per cent. from each other. The greatest difference occurs in 1881 and is 6 in the last digit, or 0·6 per cent. For practical purposes they lead to the same results: and what they show is this:

In 1881 the birth-rate gave a greater fall than the fertility: the indication of the rate on married women 15–45 was about right.

In 1891 the birth-rate, considered as a measure of fertility, again gave too low a figure: the indication of the rate on married women was also rather low.

In 1901 the figure given by the birth-rate was about right, that given by the rate on married women much too low.

In 1911 the figures given both by the birth-rate and by the rate on married women were too low.

It will be seen then that not even the proportion of births to married women under 45 years of age can be entirely trusted as a measure of fertility. In the years immediately preceding the war it tended to give a slightly exaggerated idea of the fall: some standardisation based on the age-distribution of the married women is essential.

We may take it, then, that the fall in fertility of married women between 1871 and 1911 was some 28 per cent., and that up to 1911 at all events the fall was taking place at accelerating speed. Reckoning the percentage in each case on the value of Tait's coefficient at the beginning of the period, the fall in each decade was as follows:

	Census years	Three years' average
1871–81	0·5 per cent.	0·5 per cent.
1881–91	5 „ „	7 „ „
1891–01	12 „ „	10 „ „
1901–11	14 „ „	14 „ „

The value of the birth-rate in 1891 was exceptionally high, forming a sharp peak on the curve; if a three years' average is taken of the births round the census years, the approximate figures of the second column are obtained and are probably the better representation of the course of affairs. They show a rapidly accelerating decrease.

We may then add to our conclusions:

(7) The main factor in the fall of the birth-rate has been a decrease in the fertility of married women: this fall has been proceeding at an accelerating speed.

This discussion fairly clears up the facts so far as England and Wales as a whole are concerned. But before going further it may be as well to devote a few lines to the further consideration of the method of calculation adopted. Note that there are several matters which it does not take into account. Any possible effect of age of the husband is ignored, and likewise the duration of marriage—the number of children to be expected in a year from wives aged, say, 35 is not the same if they have been married 20 years as if they have been married 5. But age of husband is relatively a very minor matter; Dr Dudfield (refs. 8) included age of husband in a method of standardisation and its effect was quite immaterial. It must be remembered that in correcting for age of wife we are in effect making a partial correction for age of husband, since the two are very closely related, and the observed fertility rates for married women of a given age are dependent, in some degree, on the age of their husbands. We standardise, in fact, to wives having husbands of the age usually associated

with their own. Our fertility rates used for standardisation do not represent a law of nature for women, but only effective fertility in certain circumstances, and there is an element of arbitrariness—as indeed there is in all methods of standardisation.

As regards duration of marriage, in addition to age, it is difficult to see any possible method of correction, and, it may be added, the data as to the effect do not seem to be consistent.

There is another possible source of discrepancy, in the earlier part of the period, for which Dr Dudfield also attempted some allowance, namely omissions to register. Such omissions will have been greatest at the beginning of the period considered, and will have tended to make the apparent fall in the decade 1871–81 too small. A correction is, however, very hypothetical.

The next question that presents itself is this: has the fall in fertility affected all classes equally, or has there been any differential incidence on the various social classes? The answer is, almost certainly, that the fall has affected successive social strata from the top downwards in a rapidly decreasing degree.

There can be no doubt that at the *present* day the upper classes and unskilled labour stand at opposite poles as regards fertility, though the evidence is scrappy and indirect owing to the imperfections of our registration system. I give in Table VI some evidence from the Report of the Registrar General for 1912.

The births in 1911 were classified according to the occupation of the father, which has to be stated at the time of registration. These occupations were grouped into five main classes, 1 and 2 representing broadly the upper classes and middle classes and 3 to 5 the manual working classes, graduated from skilled

workmen down to unskilled labour, apart from three special classes separately tabulated, viz. textile workers, miners, and agricultural labourers. The detailed classification was, necessarily for comparative purposes, that of the census. The subsequent grouping under heads 1 to 5 especially is necessarily rough since many of the census groups are very heterogeneous, pooling together persons of very different grades in the same industry or profession—but it probably serves well as a broad indication of the way things are going. We have to allow, however, for the additional difficulty that the occupation given at the registration of the birth may often have been different from that given on the census schedule and the difference might create some discrepancies.

Table VI.

From Report of Reg. Genl. of England and Wales, 1912. Births classified by occupation of the father. Classes 1 and 2 represent preponderantly the Upper and Middle classes; 3 to 8 the Manual Working classes graduated from skilled to unskilled; 6, 7 and 8 are special groups.

	Legitimate births in 1911	
Class	per 1000 males, 10 years and over, including the "retired"	per 1000 married males under 55, including the "retired"
1	47	119
2	46	132
3	73	153
4	70	158
5	90	213
6 (Textiles)	50	125
7 (Mining)	107	230
8 (Agriculture)	49	161
3 to 8	76	175
All Groups	62	162

Two rates were then calculated from the births so classified (1) the rate per thousand males in the same occupation, 10 years of age and over (without respect to their being married or otherwise), and (2) the rate per thousand *married* males in the same occupation, under 55 years of age—including in each case those classified as retired from a given occupation with those still occupied therein. Taking the first column, we see that the rates for classes 1 and 2 do not differ appreciably: there is an abrupt rise to class 3 (skilled labour), a hardly material fall to class 4, and then again an abrupt rise to class 5 (unskilled labour). Textile workers and agricultural labourers show very low figures, little above those for the upper and middle classes: miners swoop above all others.

These figures are a measure, not of married fertility, but of the fertility of the class as such: if it marries little or late its fertility thus measured will be low. The figures of the second column are an approximate measure of married fertility, though not a good measure as it is impossible to make any correction for ages of wives. In this column the rise of the fertilities from class 1 to class 5 is continuous and unbroken: textile workers would fall in between classes 1 and 2; agricultural labourers would come a good deal lower on the scale (higher in fertility) than on the figures of the first column, in between classes 4 and 5; and miners, as before, would stand well at the top. With all their imperfections I do not think the figures, even if they stood alone, would leave much doubt that there were great differences between the social strata as regards married fertility; that occupational differences, e.g. as between textile workers and miners, were sometimes quite as large; and that the differences between married fertility were often emphasised by differences between the tendency to

marriage—compare for example the pairs of figures for class 1, for miners, and for the class of agricultural labourers.

Interesting though these figures are, they do not throw any light on the question whether such differences between social or occupational classes have been increased by the fall in the birth-rate: they only show that striking differences at present exist. Table VII of which the data respecting 1871 and 1901 have

TABLE VII.

Fertility in London.

District	Female domestic servants per 1000 1901	Tait's coefficient 1871	1901	1911	Percentage change 1871 to 1901	1901 to 1911
Hampstead	166	1·52	1·06	1·11	−30	+ 5
Kensington and Paddington	145	1·50	1·17	1·09	−22	− 7
Chelsea	112	1·40	1·16	1·09	−17	− 6
St Marylebone	109	1·43	1·33	1·05	− 7	−21
Lambeth	41	1·61	1·33	1·15	−17	−14
St Pancras	37	1·42	1·19	1·17	−16	− 2
Islington	36	1·57	1·24	1·17	−21	− 6
Camberwell	33	1·62	1·34	1·21	−17	−10
Southwark	17	1·47	1·42	1·36	− 3	− 4
Poplar	14	1·62	1·60	1·51	− 1	− 6
Bermondsey	14	1·58	1·57	1·43	− 1	− 9
Shoreditch	13	1·43	1·52	1·53	+ 6	+ 1
Bethnal Green	12	1·59	1·60	1·50	+ 1	− 6

been extracted from a table in my paper of 1905 (ref. 6), does something towards giving an answer. It shows the values of Tait's coefficient, as a measure of fertility, for thirteen districts of London in 1871, 1901 and 1911. It was unfortunately impossible to give more districts because in 1871 occupations were classified by Registration Districts, and in 1901 and 1911 by Metropolitan Boroughs, so a comparison could only be made when the Borough was coterminous with a Registration District, or with two or

more Registration Districts (as in the case of Kensington and Paddington). The figures for 1911 must also be used with a good deal of caution, since in that year for the first time births in lying-in institutions were as far as possible transferred to the parents' areas of residence: this alteration will have largely affected St Marylebone, the area in which Queen Charlotte's Lying-in Hospital is situated, and probably the irregularity of the data is to be ascribed to that cause*.

The districts are arranged in the order of the number of female domestic servants employed, per thousand of the population, in 1901, taking this as a measure of the social standing of the district, so that the fertilities of upper-class and lower-class districts can be compared. It will be seen at once that in 1871 the differences were by no means regular or striking. The fertilities of Hampstead and of Kensington and Paddington, for example, exceed those of Southwark and of Shoreditch, in spite of the former standing at the top of the list in order of social scale, and the latter at the bottom. In 1901 matters were quite different. The districts at the top of the list show very low values of the fertility coefficient, while those at the bottom have practically maintained the values of 1871. In 1911 this result is, if anything, clearer: the values of the fertility coefficient run rather more uniformly from top to bottom. Contrasting the three districts at the top (avoiding Marylebone) with the five at the bottom, the average values of k run

	1871	1901	1911
Top three	1·47	1·13	1·10
Bottom five	1·54	1·54	1·47

* There were 219 legitimate births in the hospital in 1871, and 662 in 1901, and this increase will have contributed to the stabilisation of the fertility in 1871–1901.

In the thirty years 1871–1901 the five districts at the bottom of the list exactly maintained their fertility: the three at the top dropped their fertility by 23 per cent. During the last decade the top districts on the average have decreased their fertility by some 3 per cent., the five bottom districts have decreased theirs by nearer 5 per cent. or very little more. The figures for individual districts are, however, so erratic that I should not like to lay any stress on this last result. It seems probable enough that the more rapid decrease may have spread from the upper strata downwards, and in the decade 1901–1911 have begun to affect even such districts as Poplar, Bermondsey and Bethnal Green, but more evidence is necessary before this can be accepted as a demonstrated conclusion. Possibly, for example, the districts were themselves changing in character.

Dr Heron (ref. 7) in his paper of 1906 reached similar conclusions on very similar data, for he also took the districts of London as the basis of his work. He discussed, for 1851 and for 1901, the correlation of the legitimate birth-rate on married women over 20 years of age with various measures of the poverty or social grade of the district such as the proportion of professional men, of domestic servants, of pawnbrokers, of general labourers, etc., and found on the whole (though with exceptions in certain cases) a very marked decrease in the closeness of the correlation in 1851 as compared with 1901.

So far as I know no progress was made on this particular point since the date of Heron's and my own papers until the present year, when a paper by Dr Stevenson on the fertility data collected at the census of 1911 emphasised it once more. I extract in Table VIII a portion of one of his tables (the upper half of Table II). This table shows, for the

same occupational classification as is used in Table VI above, the total fertility of each class (the average number of children to a family whether surviving or no) as a percentage of the average fertility for all classes together. The marriages are classified according to duration, and the fertilities, before reducing to percentages, were "standardised" by a process similar to that already described for the birth-rate in

TABLE VIII.

(From Stevenson, ref. 12.) Standardised total fertility of marriages of various dates in each social class (cf. Table VI), per cent. of the corresponding rates for occupied persons of all classes jointly.

Date of marriage	Duration years	Social Class 1	2	3	4	5	Textile 6	Mining 7	Agriculture 8
1906–11	0–5	80	92	98	102	114	87	120	114
1901–06	5–10	79	91	98	101	112	86	122	114
1896–01	10–15	76	89	99	101	114	86	125	114
1891–96	15–20	74	88	99	101	113	88	127	115
1886–91	20–25	74	87	100	101	112	90	126	114
1881–86	25–30	76	89	100	101	110	92	124	114
1871–81	30–40	81	93	101	101	107	93	117	109
1861–71	40–50	88	96	101	100	104	94	113	104
1851–61	50–60	89	99	101	99	103	94	108	105

order to eliminate the effect of varying age of wife at marriage. Confining our attention first to the classes 1 to 5 which run in descending order down the social scale, we see that in the case of the marriages of 50 to 60 years duration which took place in the decade 1851–61, the fertilities only range from 11 per cent. below to 3 per cent. above the general average, and the figures do not run quite regularly. Class 1 is clearly, even then, below the general mean; class 2 is only just below, and class 3 is just above; class 4 is just below again and class 5 just above: the lowness of fertility for the professional class is, in fact, the

only marked feature. But looking at the more recent marriages, say those of 10 to 15 years duration which took place in the quinquennium 1896–1901, we see quite a different state of affairs. The fertility of class 1 is now not 11 per cent. but 24 per cent. below the general average: that of class 2 is 11 per cent. below: that of class 3 is just below and of class 4 just above: that of class 5 is 14 per cent. above. The differentiation is quite regular and much more emphasised. In the marriages of under ten years duration which have taken place since 1906 the differentiation seems again to be slightly diminishing but, as pointed out by Dr Stevenson, this may perhaps be due to the fact that in a period of less than ten years the more fertile marriages have not had time fully to exhibit their fertility.

The data for the special classes, (6) Textile workers, (7) Miners and (8) Agricultural labourers are also very interesting. Even in the marriages of 1851–61 the total fertilities of these classes are clearly differentiated, standing in the ratio of 94 : 108 : 105. The figure for textile workers stands between those for classes 1 and 2—an extraordinarily low figure for the working class—while the figure for miners exceeds that for unskilled labour (class 5). In the marriages of 1896–1901 the differentiation had become very much greater, the percentages being 86, 125, 114: but both the statements as to relative position remain true. The figure for textile workers is still between the figures for classes 1 and 2, and the fertility of miners still exceeds that of unskilled labour. Both conclusions, it should also be noted, are confirmed by the births for 1911 as tabulated in Table VI on the married males under 55.

The percentages given in the table are based on total fertilities, i.e. on the total numbers of children

born. If we take instead the figures for effective fertilities, i.e. for numbers of children surviving at the date of the census, there are some interesting changes but no alteration in the essential point as to the social gradation at the present day. The figures for effective or net fertilities are given by Dr Stevenson in his table and they compare as follows with those for the total fertilities. For the marriages of 1851–61:

	Class							
	1	2	3	4	5	6	7	8
Total	89	99	101	99	103	94	108	105
Effective	91	102	98	98	99	91	97	111

For the marriages of 1896–1901:

	Class							
	1	2	3	4	5	6	7	8
Total	76	89	99	101	114	86	125	114
Effective	81	92	99	101	109	83	119	119

In the marriages of 1851–61 the only marked feature in classes 1 to 5 is, as before, the lowness of the fertility of the professional class: in net fertility, however, class 2 stands higher than class 5, thus reversing their positions for gross fertility. But the difference either way is small. In the case of the special classes it is interesting to note that the positions of the miner and the agricultural labourer are reversed: the net fertility of the agricultural labourer is considerably higher than that of the miner.

Turning to the marriages of 1896–1901 we find that the substitution of effective for total fertility leaves classes 1 to 5 in precisely the same order as before, and only reduces the range. The total fertilities of classes 1 and 5 stand in the ratio of 76 : 114; their effective fertilities in the ratio of 81 : 109. The gross fertility of unskilled labour exceeds that of the professional classes by some 50 per cent.: its effective fertility exceeds that of the professional classes by

35 per cent. In the case of the special occupational groups the main alteration has been to equalise the figures for miners and agricultural labourers. The miner loses by excessive mortality the initial excess of his number of children over that of the agricultural labourer. It must be remembered that the duration of these later marriages is much less than that of the earlier marriages and so mortality has had less time to operate. But the mortality under age 10 is some two-thirds of all the mortality up to age 40, and the difference is, therefore, not so great as it appears.

In the light of these tables—or rather of the much fuller discussion in the papers from which they have been taken—we can, I think, add to our conclusions:

(8) At the present date there is no doubt that marriage fertility is on the whole, broadly speaking, graduated continuously from a very low figure for the upper and professional classes to a very much higher figure for unskilled labour.

(9) At the same time there are some very marked occupational differentiations which cut right across the social gradation.

(10) The social and occupational differentiation is now very much greater than it was half a century ago. There is hardly sufficient evidence to show whether it is again decreasing.

Now on the broad facts as derived by this analysis I do not think there is any disagreement. On the interpretation of the facts there is some disagreement. The view usually taken is that the decline in fertility of married women is "due to increasing practice of contraceptive measures" (Stevenson, ref. 12). Dr Stevenson argues that only this theory is consonant with the fact that the decline apparently operated first on the more prosperous and educated classes, and that only this theory can explain the actual date of

the beginning of the fall as it corresponds in time with the opening of the Bradlaugh-Besant propaganda. Those who hold this simple theory in its purest form seem to think that in murmuring "contraceptive measures" they have explained everything.

Personally it seems to me to be a theory which even if true explains nothing. If someone asked why so many young men came to Cambridge about the middle of January, he would hardly be satisfied by the information that they came in trains. Contraceptive measures are a means not a cause.

Have we any reason for supposing that they have been even the primary means? The only definite evidence we possess directly negatives the idea. Certain information was obtained by the Birth-rate Commission (ref. 11, pp. 323–4) as to size of family and limitation. On 167 schedules the question as to limitation was unanswered: limitation was acknowledged by 289 and denied by 188. Excluding childless families, the average number of children in "unlimited" families was 2·5, in "limited" families as a whole 2·6, in families limited by artificial means (as distinct from abstinence, which ought not to be confounded with them) 2·5. There is no appreciable difference. But, it may be said, this shows perhaps only that the "unlimited" families are naturally the less fertile: on the contrary they come from slightly the more fertile stocks. The number of children in the parental families averages 6·1 for the unlimited, 5·5 for the limited.

The basis here is small but the conclusion is in accord with the evidence of other samples, even smaller though these are. Some schedules obtained by Mr Sidney Webb show an average of 2·88 children for "unlimited" marriages of about 18 years duration; 2·7 for "limited" marriages of 14 years dura-

tion (*ibid.* p. 26). In yet another small sample 19 schedules for "unlimited" families gave an average of $2\frac{1}{2}$ children; 75 for "limited" families an average of $3\frac{1}{8}$ (*ibid.* p. 330). Surely, in view of these data, it cannot be pretended that a reference to contraceptive measures goes far to explain the fall in fertility or even indicates the chief means by which it has been effected?

But, it may be said, have we any evidence to show that at other times or in other countries a decrease in fertility has taken place apart from the use of contraceptive measures of an artificial kind? It is not necessary to find a decrease in fertility: if we find an increase, and can be confident that in the earlier year contraceptive measures were not practised, we have shown all that is necessary, viz. that a low fertility may occur without the necessary implication of artificial methods. Dr Stevenson himself cites a very striking case of the kind, without, as it seems to me, realising its logical consequences. The case is that of Connaught (ref. 11, pp. 350–1). In Table IX are

TABLE IX.

Fertility in Connaught.

Year	N. and S. standardised birth-rate (1)	Tait's coefficient (2)	Comparison with 1871 (2)	Comparison with 1881 (1)	Comparison with 1881 (2)
1871	—	1·83	1000	—	—
1881	33·60	1·72	940	1000	1000
1891	—	1·92	1049	—	1116
1901	37·98	1·95	1066	1130	1134
1911	45·3	2·39	1306	1348	1389

given standardised legitimate birth-rates for Connaught calculated by the method of Newsholme and Stevenson and given by them in ref. 5 and ref. 11, and also the values of Tait's coefficient calculated by myself for the sake of comparison over the whole

series of census years from 1871 to 1911. Calling the fertility 1000 in 1871 it fell to 940 only in 1881; rose again to 1049 in 1891, and to 1066 in 1901 and then swooped to no less than 1306 in 1911. These figures are based on my calculation of Tait's coefficient. Newsholme and Stevenson gave standardised legitimate birth-rates for 1881, 1901 and 1911 only and I have not calculated the figures on their basis for the other years. Comparing the two series, however, for the three years for which both are given, so as to check the startling result for the last intercensal decade, it will be seen that we are in practical agreement in 1901 and differ by 3 per cent. only in 1911. We are then safe in concluding that the fertility of these Irishwomen was 35 to 39 per cent. greater in 1911 than in 1881: it was also some 6 per cent. greater in 1871 than in 1881. It is agreed that contraceptive measures in this case may be ruled out of consideration, Connaught being an almost purely Roman Catholic district and the Church setting its face sternly against their use. What then was happening in 1881? It is true that even then the fertility, as measured by Tait's coefficient, was greater than that of the married women in the lowest-class districts of London, but still it was far under the fertility of which the Irishwomen were capable. Surely only one conclusion is possible; that a fertility under the maximum of which women are capable is not to be regarded as an unnatural phenomenon, but as a perfectly natural consequence of natural causes. This one, but remarkable, instance is, as it seems to me, sufficient to rule out the idea that the use of artificial methods of preventing conception is necessarily involved when fertility is low.

If we want evidence from earlier times before 1875 the chief difficulty is the lack of trustworthy statistics.

If we could trust the birth-rate as an index there would be little difficulty. The birth-rate in France, for example, decreased not from 1876 or thereabouts but from the very beginning of the nineteenth century onwards: in 1811–20 the average rate per thousand of the population of all ages averaged 31·8: in 1841–50 it had fallen to 27·4 or by 14 per cent. In 1871–80 it had fallen further to 25·4 (cf. Table II) or by a further 7 per cent. and in 1901–10 by a yet further 19 per cent. The fall of the birth-rate was not a phenomenon dating only from the seventies. Again, the birth-rate in Geneva averaged as follows over certain periods (data cited from Brownlee, ref. 9):

1695–1710	36·8
1711–1730	33·4
1731–1750	31·5
1751–1770	33·3
1771–1791	31·2
1806–1812	24·3
1814–1823	20·8
1824–1833	22·4

Between the beginning of the eighteenth century and the early nineteenth century there was a fall of over 40 per cent. For our own country (England and Wales) we have no early records of the birth-rate. But the best estimates of population in the eighteenth century suggest an increase from 5·8 millions in 1700 to 6·3 in 1750, an increase of only half a million or 9 per cent. At the end of the century the population was some 8·9 millions, an increase of 2·6 millions or 40 per cent. (cf. Gonner, *Journ. Stat. Soc.* vol. LXXVI. 1913).

But, however suggestive these figures may be, we cannot trust the birth-rate as an index to married fertility for we cannot be certain that it has not been materially affected by changes in the proportion of married women to the population, and the rate of increase of a population is affected by the death-rate

as well as by the birth-rate. In Table X I have given a few series of figures showing the legitimate births per thousand married women between the ages of 15 and 50. Sweden affords the only long series. Here we see that the decennial averages fell from 251 in the decade round 1760 to 232 in the twenty years round 1805. The figure rose again to 253 in 1816–25, only to fall once more to 235 in 1836–45. It then fluctuated up and down through the next few decennia and in 1886–95 was only just below the value noted for 1796–1815. The fluctuations, prior to the decennium ending in 1905, were only of the order of 8 or 10 per cent.—but the rate cannot be said to have been constant.

TABLE X.

Legitimate births per 1000 married women aged 15 to 50.

	Sweden	France	Belgium
1756–1765	251		
1766–1775	240		
1776–1785	242		
1786–1795	245		
1796–1805	232		
1806–1815	232		
1816–1825	253		
1826–1835	240		
1836–1845	235		
1846–1855	241	179	252
1856–1865	248	172	276
1866–1875	235	172	270
1876–1885	240	167	264
1886–1895	231	150	236
1896–1905	219	134	213

The averages for France cannot be carried back beyond 1846. The excess of the average for 1846–55 over that for 1856–65 is not great (about 4 per cent.) but so far as it goes confirms the suggestion of the birth-rate that the fall in fertility began long before 1875.

In the case of Belgium there was a sharp rise of 10 per cent. from 1846–55 to the following decade, and a steady fall thenceforward. So in this case there are two interesting points. In the first place the fall began before 1875. In the second the fertility of the decade 1846–55 was lower than that of any subsequent decade before 1886–95. As in the case of Connaught the question arises as to the cause of this low fertility in the early period: France showed then a fertility higher than that of the following decade.

No doubt one would like to have much more extensive data reaching much further into the remote past, but so far as they go these figures do confirm our previous conclusion that fertility cannot be regarded as rigidly fixed—would it not be a most amazing thing if it were rigidly fixed? a phenomenon almost without parallel?—but that it may exhibit depression in countries and at times when it is improbable that artificial methods of contraception can be adduced as an explanation.

Personally, then, I think we can clearly conclude:

(11) The recent fall in fertility has not been effected solely or mainly by the use of artificial methods of contraception. The only definite data we possess are against this view.

(12) In general, fertility cannot be regarded as a fixed quantity for a given nation, but is subject to natural fluctuations.

As regards the nature of such fluctuations two views have been advanced. Dr Brownlee (ref. 9) has advanced the view that the fluctuations are physiological in character, rhythmic variations (not of any fixed period) in "germinal vitality," analogous to those outbursts of vital energy which lead in the case of an infective organism to epidemics, in the case of higher forms of life to such phenomena as plagues

of field-mice, or plagues of locusts. On this hypothesis (*loc. cit.* p. 22) "the birth-rate is not dependent in general on the immediate conditions which surround the organism, but upon conditions which probably precede the period of high birth-rate and which are favourable to the storage of the specific energy." We know nothing whatever of the causes which lead to such "epidemics" of an infective organism or of higher animal forms: we only know that they do occur, and there is no reason why man should be exempt from analogous phenomena. Dr Brownlee endeavours to relate periods of rapid increase in nations with other symptoms of energy, e.g. of remarkable literary output and of "racial adventure." I must refer to his original paper for the detailed argument, and to his evidence before the Birth-rate Commission where the hypothesis was supported by Dr Chalmers.

At the end of his paper Dr Brownlee states that, in advancing such a hypothesis, he does not mean to discount the direct influence of environment in producing an increase of population, but apparently he would regard any such effect as in general subsidiary. In my own investigation of 1905, however, I endeavoured to trace the effect of definite causes on the birth-rate. First of all I endeavoured to see if there was any direct response to the economic wave—the short-period movement usually some 7 to 11 years from crest to crest which is so marked a feature in the course of trade, or of prices, or of almost any index to economic activity. The problem is not an easy one, for we have first to estimate the indirect effect owing to the influence of the economic wave on the marriage rate. Calculations showed that the actual oscillations in the birth-rate were nearly twice as large as they would be if they were merely the indirect effect of

the waves in the marriage rate. It seems clear then that the birth-rate does show a direct, as well as an indirect, response to the economic wave.

Is there any economic variable that can be suggested as apparently possessing a dominant influence on the secular trend of the birth-rate? The answer was suggested to me by the work already done on the marriage-rate. In that case I showed that prices were the only economic variable (so far as I could find) which fulfilled the essential conditions and that it fulfilled them very well, but that after 1885 or thereabouts some other dominating cause seemed to be in operation. Precisely the same conclusion holds for the birth-rate—or rather let us say for the course of fertility. If we draw a curve showing for each year the average of, say, Sauerbeck's (the Statist) index-number for that year and a few years preceding it we get a curve very like that showing the course of fertility, but the latter fails to respond to the rise in prices which took place from 1896 onwards: as in the case of the marriage-rate some other dominating factor seems to enter in. That the course of prices is closely related to the trend of the marriage-rate and of fertility I am as convinced now as I was in 1905, but am equally at a loss to suggest the precise nature of the nexus. That the nexus is economic, and that it probably operates via psychology rather than directly through physiology is my view; and I doubt —in fact I disbelieve—its being wholly conscious, or as the phrase now goes "volitional."

In the paper to which I am referring I illustrated further the apparent influence of prices by comparing the course of prices during the nineteenth century with the course of population. That comparison can now be carried on to one additional intercensal decade. I have plotted on a rough chart a line show-

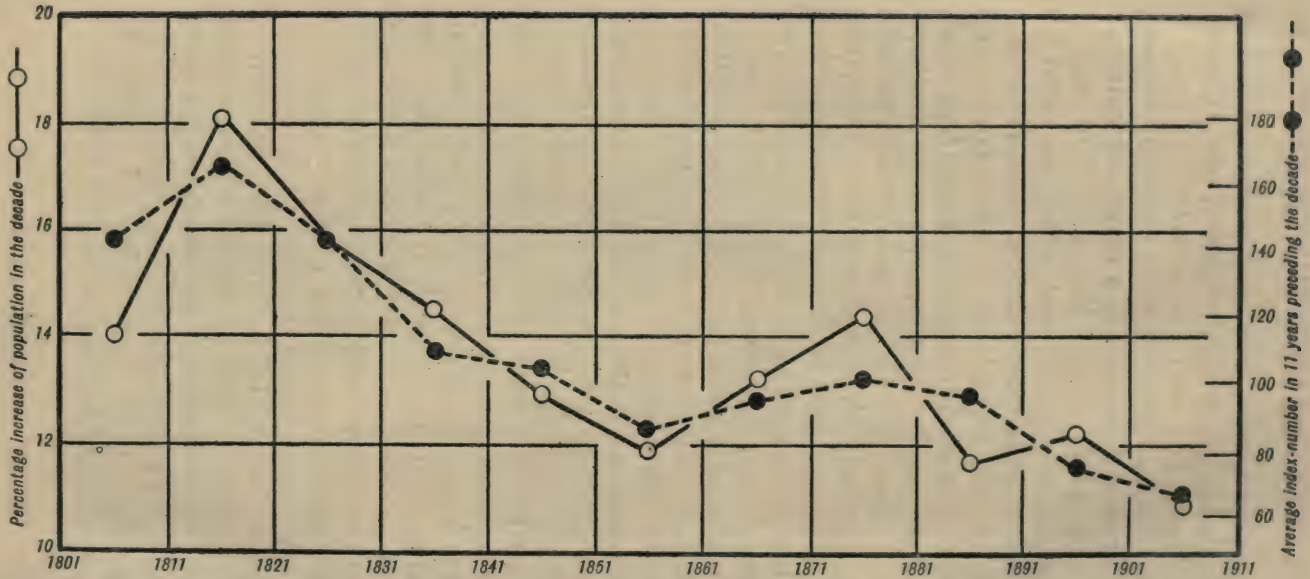

Chart showing the intercensal percentage increase of population in England and Wales, for each decade from 1801–11 to 1901–11, compared with the average price-level in the eleven years preceding that intercensal decade.

The intercensal increases of population are given by the full line and the scale on the left: the price-level (1867–1877 = 100) by the dotted line and the scale on the right, the scale and base-line being so adjusted as to make the two curves fit as closely as possible.

ing the successive intercensal increases of population in England and Wales, and another giving the 11-year average levels of prices (as indicated by the index-numbers of Jevons and Sauerbeck), the latter having its base and its scale so adjusted as to fit the population-increase curve as closely as may be and also being shifted by ten years: i.e. the point in the centre of the decade 1801–1811 gives the price level of 1791–1801 and so on. The two curves seem to me to be too similar for the similarity to be accidental. The only time when the two are positively discordant is in 1881–1901 when the rate of increase rose but the price-level (of a decade earlier) fell*. Considering how complex a quantity the rate of increase of a population is, and how dependent nowadays on the rates of emigration and of immigration as well as mortality, it may well seem astonishing that any similarity at all should be evident. That there is such resemblance is, I venture to think, due to the fact that migration, marriage-rate, and fertility are only three forms of response to demand for population: if part of the demand is unexpectedly met by a lowering of the death-rate fertility may well be checked. But let me again enforce a point which seems to me of importance. The demand is for adults. A rise in the birth-rate responds by the production of infants who are not available for economic production for some twenty years or more. Population —workers—are demanded now: workers turn up some twenty years later, when they may not be wanted. And, if there is any truth in this economic theory of population, this lag in response must tend

* For those who are accustomed to the use of the coefficient of correlation as a measure of resemblance, it may be added that the correlation between the intercensal increase-rate and the index-number of the eleven years preceding the decade is 0·91: the correlation between the first differences (movements) of the two quantities is 0·72.

to give rise to a swing in population of a very long period.

On the general question of the relation between rate of increase of population and prices, it may be pointed out that the course of population in England and Wales during the eighteenth century, to which reference has already been made, seems to be in general accordance with the same rule. In the first half century, when the price-level was low, population increased very slowly, at the rate of less than 2 per cent. in the decade. During the third quarter of the century, when prices were little higher, the rate of increase was still low, averaging under 4 per cent. in the decade. During the last quarter prices had risen to a much higher level and the rate of increase of the population averaged over 10 per cent. per decade.

The war has effected an unprecedented break in the life of our own nation, and of all European nations, and in the economic conditions of the world. The course of many things in the future will be of the highest interest, and the course of the birth-rate not be the least interesting. You will see from Table I that the figure for 1919 showed some recovery from the war level, but the average for the year hardly indicates what was happening. The figures for successive quarters since January, 1919, have been as follows:

1919	First quarter	15·6
	Second quarter	15·9
	Third quarter	18·5
	Fourth quarter	23·6
1920	First quarter	29·0

It was not till the third quarter of 1919 that the post-armistice conceptions began to make themselves felt. The usual course of the birth-rate after a war is a rapid rise to a quite abnormal figure in the first year

or two following, and then a relapse to normal. It will be several years before we can tell how things are really going: obviously we cannot expect that a rise of prices so cataclysmic in character as that which took place during the war will be accompanied by changes in population falling within the rule shown by the chart of population and prices. The whole situation has been changed.

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For international comparisons *Statistique internationale du mouvement de la population*, vol. I. jusqu'en 1905, 1907; vol. II. 1901–10, 1913 (Paris Statistique générale de la France) will be found useful.

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